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ABSTRACT

The author is concerned with the role of preconception in the process of creative thinking, and as a psychologist, he experimented to determine what variables influence creative thinking. In the process of experimentation, however, he began to feel that some specific preconceptions were warping his investigation and were detrimental to a useful study of creativity. In this report he reviews the various modes of approaching analysis of creative cognition, pointing out their shortcomings, in particular isolating variables of behavior rather than identifying systems of behavior. The implication is that modern data gathering methods are tailored to the method of analysis, not the content under analysis. The author emphasizes the need for more creative "cognitive maps," internalized modes of thinking, to be brought to the question of analyzing creative intelligence. Prefatory remarks by Manuel Barkan relate the author's presentation to the interests of the art education profession. (JM)

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CREATIVITY AND THE PREPARED MIND

by Ray Hyman, University of Oregon

Sponsored by the Viktor Lowenfeld Memorial Fund

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The NATIONAL ART EDUCATION ASSOCIATION
1201 16th Street, N.W., Washington, D.C. 20036 / 1965

INTRODUCTION

The writings and the influence of Viktor Lowenfeld are in themselves a lasting and incontestable memorial to the man, assuring him a place of pre-eminence in the history of art education.

At the time of his death, however, a memorial fund was established under a national committee. Fitting utilization of interest from this fund was under discussion during the planning for the NAEA 1963 Convention.

It was then determined to sponsor at the National Convention a Lowenfeld Lecture, which was to be purposefully oriented toward new research and new influences of the widest variety which held promise for art education. Thus such a lecture would express symbolically the combination of awareness, openness, and scholarship typical of Lowenfeld himself. It was further hoped that these lectures might be published and accumulate into a significant and vital series of art education monographs.

In this spirit, Ray Hyman was invited to give the first Lowenfeld Lecture. His research and his bold fresh thinking on the experimental study of creativity well qualify him to speak to the issue he shares with us in the text to follow. Commenting on Hyman's paper, Barkan perceptively delineates its relevance to research in art education.

It is thus a pleasure to introduce this first of what I believe will be a distinguished series of monographs.

KENNETH R. BEITTEL
Pennsylvania State University

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PREFACE

MANUEL BARKAN

Reading Ray Hyman's paper on "Creativity and the Prepared Mind" is at once a reaffirming and a refreshing experience. His concern with the paradoxical character of the prepared mind is a reaffirmation of one of the perennial problems and realities of human perception. His confrontation of himself, as an inquirer into the nature of creative activity, with the dilemma brought about by the paradox of the prepared mind, is sufficiently refreshing for it to challenge each of us who professes to inquire into the dimensions of creativity in artistic activity.

The paradox of the prepared mind enjoys such an everlasting level of significance that it warrants the continuous attention of each and every one of us. We must know in order to perceive what is meaningful. And yet, what we think we know can often cause us to filter out of our perceptions some of the truly striking elements in experience. On the one hand, according to Hyman, the "cognitive maps" which each of us carries in his personal baggage "provide the framework for evaluating the significance of a new observation." On the other hand, however, these very same cognitive maps can act as preconceptions that "blind you to new connections or rearrangements of your cognitive material." Our desire to categorize and organize the stimuli we encounter in our here-and-now experiences often leads us to overlook those unique and rough edges in experience which do not fit our preconceptions.

Reading Ray Hyman's reaffirmation of this paradox, I am reminded of Samuel I. Hayakawa's preface to Gyorgy Kepes' Language of Vision *(1). According to Hayakawa, the very language sytem we use belies our conceptual images. Hayakawa wrote, "Whatever may be the language one happens to inherit, it is at once a tool and a trap. It is a tool because with it we order our experience, matching the data abstracted from the flux about us with linguistic units [and], having matched the data of experience with our abstractions [cognitive maps], we manipulate those abstractions, with or without further reference to the data, and make systems [preconceptions] with them. . . . [It is] a trap because languages select, and in selecting what they select, they leave out what they do not select." Hayakawa finally concludes that "every language leaves work undone for other languages to do."*

If we would grant Hayakawa his point that language and language usage are selective because they exclude attention to certain stimuli in here-and-now experience to the degree that they include attention to other stimuli, then it seems to me that we are confronted with an inescapable corollary: Languages not only are selective, but their degree of selectivity increases in direct relationship to their level of sophistication. The more sophisticated the language, the more incisive it is and the greater is its capacity for refinement and depth. At the same time, however, a more sophisticated language can also become more limited in its possibilities for range and scope. Such a language is more discreet and more restricted precisely because it has been more selectively directed toward a particular and specialized area of concern.

If we would now admit into the broad category of language the variety of visual and verbal symbol sytems, including the linguistic and statistical constructs which comprise the current psychological tools, then, I believe, we find ourselves face to face with the dilemma which confronts Ray Hyman in this

paper. He describes to us how he came to the point where he "had to violate" some of his preconceptions in order "to adequately do justice to the phenomena that seemed to be occurring" in his laboratory. If I understand him correctly, he is saying that the language system and their ideological constructs, which are embodied in the current tools now being employed by psychologists in their investigations of creativity, are quite limited in the kinds of information they can provide. He asks us to "realize that psychologists entered the field of creativity already committed to standard research designs and fixed routines for collecting and analysing data." He further points out that, "The very model and method of data reduction which is the heart of these methods removes that aspect of adaptive behavior which involves sequential dependencies among the various activities and their interactions with the environmental outcomes that they bring about." In short, the highly sophisticated and refined linguistic system which comprises the current tools for psychological investigation, though valuable in many respects, appears to be directed toward and attuned to abilities and attributes isolated and separated from the open-ended situation in which the goal-seeking individual may or may not be behaving creatively.

It is small wonder, then, that Ray Hyman laments the "aloofness" of the psychologist from the phenomena of creativity that results from his application of current psychological tools to the study of creativity. It is small wonder that he makes a strong plea for simplicity and directness. It seems to me that, among other things, Ray Hyman is calling for intimate contact with the phenomenological dimensions of creativity, for reduction of preconceived methodological interventions between the investigator and the data he can detect about the nature of creativity. It is indeed both refreshing and encouraging to hear him state the case "for naivete in the study of creative thinking;" for the wisdom of "the non-psychologist who is interested in creativity to turn away from the psychologist and psychological methodology and, rather, turn towards the phenomena of interest."

Ray Hyman draws his own moral from the story he tells, which is "to look carefully and sytematically at creative phenomena with a minimum of commitments and with no ready-made preconceptions about how to collect and analyse the data." But there is still a further moral which, I believe, we in art education are obliged to draw. We must learn what we can from the psychologist and there is indeed much that we can learn. At all costs, however, we dare not allow ourselves to become so thoroughly seduced by language system that we forsake our own. By virtue of our intimate relationship with the phenomena of creativity, the opportunity is uniquely ours to build that "good observational store" from which more meaningful pictures of creativity can be developed. The opportunity and the responsibility is ours to develop this picture carefully and systematically, so that methodological procedures can be derived to fit the picture rather than to limit or even distort it.

In this regard, it would be well to refresh our own perceptions of the ways and means of viewing any human phenomena in experience. I want to refer here to Hadley Cantril's paper "Toward A Humanistic Psychology" (2). According to Cantril, there are four levels of complexity on which experiential phenomena may be viewed: on-going naive experience, description; focused analysis and conceptualization and abstracting for scientific specification. The on-going naive experience of the first level "is the level of immediate, 'pure' experience as experienced—unanalysed, unconceptualized, unmediated, and with no concern on the part of the experiencing individual to describe, analyse, conceptualize, or communicate his experience. This is the level where the human being is in the midst of his adaptive goal-seeking behavior. The second level, where

experience is described, is already one step removed from the experience itself. It involves some focusing, some categorization and some shift from what Cantril calls "the full orchestration" of the experience. The third level of focused analysis and conceptualization is a person's effort to "figure out" conceptually what is going on in the experience for some purpose and to resolve some problem. It involves the interpretation of elements in experience which have been focused on. The fourth level, abstracting for scientific specification, involves abstractions of elements in experience which can be made to function "without reference to any particular item of behavior that might illustrate it." Such abstractions are relatively static because they "are not affected by individual behavior and are not altered when conceptualized from the point of view of different persons."

It seems to me that Cantril's point is that these four levels of viewing experience are sequential in character, that the second, third and fourth levels are outgrowths of their immediate predecessors. When the study of creativity is viewed in these terms, it becomes even more imperative to move "gradually and step by step," as Ray Hyman advises. "A good observational store" of descriptions is one of the most solid and sure steps we need to take. With such an observational store, we should be able to make some headway toward developing sufficiently reasonable focused analyses and conceptualizations. Without conceptualizations derived from such a source, I am afraid that Ray Hyman is absolutely right in saying that "the gaps between the raw data and the end product are filled with huge, unintelligible voids."

In conclusion, I want to refer to Paul Edmonston's statement in his doctoral disseration (3) where he indicated the methods he used to gather the data: "I engaged in studio painting . . . paid introspective attention to my studio behaviors . . . recorded in a journal regularly and carefully . . . read the journal to discover what studio conditions or behaviors appeared to enhance or inhibit my productivity . . . read the journal to improve my capacity to observe and to refine my methods of recording behaviors . . . photographed paintings and studio settings in chronological order of their occurrence . . . recorded regularly [the] ideas for subsequent analysis of the data [and] indicated the potential treatment which I might give to the data."

I would suggest that the kind of methodology used by Paul Edmonston, whether directed toward one's own behaviors or the behaviors of others, has some of the promise of sufficient sure-footedness to "more adequately represent the actual forms of the phenomena" than some others which take the leap directly onto the level of abstracting for scientific specification. More research into creative behavior is urgently needed for a multitude of purposes, especially in the visual arts. We do indeed need to make haste. But, in doing so, it would be well for us as art educators to value and to exploit the footing which we hold in the visual arts rather than to lose it by forsaking it.

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[1] Samuel Hayakawa, "Introduction," Gyorgy Kepes, *Language of Vision*, Chicago: Paul Theobald, 1944, pp. 8-10.

[2] Hadley Cantril, "Toward A Humanistic Psychology," ETC: *A Review of General Semantics*, Summer 1955, pp. 278-298.

[3] Paul Edmonston, "A Methodology for Inquiry into One s Own Studio Processes," (Unpublished Doctoral Dissertation), The Ohio State University, 1961.

Creativity and the Prepared Mind:

Preconceptions in Creative Achievement and in Creativity Research *

RAY HYMAN, University of Oregon

In this paper I want to talk about the role of the "prepared mind" in creative achievement. To me, the terms "prepared mind" and "creative achievement" both refer to "cognitive structures" or "cognitive maps"—that is, they refer to the internal representations of a situation by which an individual guides his behavior. These cognitive maps include both the contents and the organization of these contents. The contents, or cognitions, consist of items of information, beliefs, attitudes, and action tendencies. The organization of the cognitive map refers to the interconnections among these contents.

A creative achievement, in these terms, is the acquisition of a new cognitive map of a given situation. Although this new cognitive map may differ from a previous one by the exclusion or the inclusion of specific contents, we are more likely to think of creative achievement in reference to those changes in cognitive maps that involve the reorganization or the formation of new connections between already existing contents or items of information.

The creative achievement, then, is a new cognitive map of a situation—a map which emerges, in part, from a prior cognitive map. This preexisting cognitive map is part of what we call "the prepared mind." It is the interaction of the prior cognitive map (the prepared mind) with new information in the present situation that transforms the original cognitive map into a new, presumably more adequate, representation of the situation. The *process* of creative thinking can thus be viewed as the *transition* from an existing to a new cognitive map in which this new map may differ both qualitatively and quantitatively from the previous one; it may contain more or different elements and it may include more complex as well as different couplings among these elements.

When we describe creative achievement as dependent upon previous cognitive structures, we encounter an interesting paradox. We can see that in one sense the previous cognitive map is necessary for the achievement of the new one. Yet, in another sense, we can see that the existence of a prior cognitive map of a situation often limits or hinders the possibilities for the achievement of a new or more adaptive representation.

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* *Paper presented at the Symposium on Research and Art Education sponsored by the Viktor Lowenfeld Memorial Fund, National Art Education Association Conference, Kansas City, March 13, 1963. The research cited in this paper was supported by a grant from the General Electric Foundation.*

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As Louis Pasteur once put it, "In the field of observation, chance favors only the prepared mind." We can reinterpret this statement by saying that your pre-existing cognitive map of a situation provides the framework for evaluating the significance of a new observation. Your existing expectancies about what is possible serve to highlight discrepancies from theory and call your attention to events that "don't fit." In this sense, cognitive maps set the stage for the development of new cognitive maps; they help you to select what is relevant and to reject what is irrelevant; they provide the bridge between the here-and-now and your past experience.

From another viewpoint, however, preexisting cognitive maps have been blamed for missed discoveries and for situations where scientists have overlooked the obvious. When talking about cognitive maps within this context of hindering creative achievement, we typically refer to "preconceptions." "Preconceptions" refer to that part of your cognitive map that places unexamined and unnecessary constraints upon the possible transformations of your original cognitive map. Such preconceptions can blind you to new connections or rearrangements of your cognitive material that will more adequately enable you to cope with a current or future situation.

In terms of this background, let me now list the points that I want to make in this paper:

1. I will focus upon that part of the prepared mind or pre-existing cognitive map that we call "preconceptions."

2. I am going to deal with preconceptions at two levels of discourse.

3. At the first level, I want to briefly describe some of my own attempts to devise experiments which would help to clarify the role of preconceptions in creative achievement. In this domain, preconceptions are the focus of my research efforts. I will briefly indicate, by means of a few sample experiments, how I have gone about trying to study this problem. However, I will deal very little with conclusions that emerge from this work.

4. Instead, I will focus upon conclusions that arise when I deal with preconceptions at a second level of discourse. These conclusions came about when I started to examine my own preconceptions rather than those of my subjects. As a result of this examination, I found myself, because of my training in psychology, examining the preconceptions that are built into the methods and procedures with which psychologists today are studying creativity. In other words, I want to discuss with you some of the implications that emerge when we try to make explicit the preconceptions that underlie current research in creativity.

5. My conclusions, in a nutshell, will be:

a. When psychologists turned their attention to creativity as a domain for research (which they first began on a significant scale in 1950), they brought with them methods and routines that were highly standardized and codified.

b. These routines had been devised and perfected earlier in the history of psychology and in response to other issues and problems.

c. As a consequence, the methods carry with them several built-in and unexamined preconceptions about what kinds of observations to make, how to summarize data, and how to analyse and interpret findings.

d. Many of these preconceptions, when brought to light, are at variance with creative thinking and achievements as most of us would describe the phenomena from either first-hand or second-hand accounts.

e. We need to carefully describe the domain to which these current procedures apply, if we are to effectivly utilize current findings.

f. More importantly, we have to carefully describe the domains and questions for which current methods are completely inadequate or inappropriate, if we are to free ourselves to go in other directions and consider alternative methods and models which may be more relevant to an understanding of creative achievement.

Creative Achievement as the Overcoming of Preconceptions

My research program began with the question: What is the role of preparation and preconception in creative achievement? Although creative achievements occur in many forms and guises, many, if not all, can be characterized as the discovery of a similarity between two previously unconnected facts. This discovery of hidden connections which comes from a re-examination of what we already "know," (what is already before us), appears to be a major theme of creative achievement in science and technology. At least that is the message that comes through to me which I read such books as Taton's *Reason and Chance in Scientific Discovery;* Jewkes' *The Sources of Invention;* I. Bernard Cohen's *Science, Servant of Man;* and the *Harvard Case Histories in Experimental Science.*

Poincare, perhaps puts the matter most simply. He views creative achievement as the production of combinations that "reveal to us the unsuspected kinship between . . . facts long known, but wrongly believed to be strangers to one another." This "unsuspected kinship," once it is pointed out, often possesses a certain obviousness. The following quotation from an Associated Press Story from Buffalo, New York is a vivid example from the non-scientific realm:

"The post office here had a problem. Mail handlers were annoyed by exhaust fumes from delivery trucks parked at a loading platform. Michael P. Gorman, one of the loaders, came up with a solution that won him $12.50 and a certificate of merit for a beneficial suggestion. His suggestion —turn off the truck motors."

Such examples cause us to look at creative achievement in terms of overcoming preconceptions or unlearning existing connections in order to find "unsuspected kinships" between already known facts. They raise

questions such as: What is it that keeps a man from fully utilizing the skills and knowledge that he already possesses? Why doesn't he more frequently see those combinations that reveal the unsuspected kinships?

Mary Henle, in a paper called "The Birth and Death of Ideas," states in a dramatic fashion, the issue that initiated my research program three years ago. In discussing "immersion in one's subject matter" as a condition for creativity, she points out that "In general, the mathematician tends to get his good ideas in mathematics, the musician in music, the psychologist in psychology. Our creative thinking tends to be in fields and in relation to problems that we know a good deal about."

But she immediately recognizes the paradox in her statement. For she has earlier pointed out that it is the commonplace rather than the unknown which is the enemy of creative thinking:

"It has already been pointed out that knowledge may work against creative thinking, for we do not think about what we know. We cannot become steeped in a field without also becoming steeped in the ideas current in that field. And existing ideas tend to blind us to new ones. It seems that creative ideas do not occur to us unless we spend a great deal of time and energy engaged in just the activity that makes their emergence most difficult."

This paradox, of course, has been recognized by many scientists. Beveridge, in his insightful book *The Art of Scientific Investigation,* after discussing the possibility that prior information "makes it difficult to find a new and fruitful approach." nevertheless concludes "that it is a more serious handicap to investigate a problem in ignorance of what is already known about it."

In an attempt to reconcile the two different views toward previous knowledge, Beveridge suggests:

"The best way of meeting this dilemma is to read critically, striving to maintain independence of mind and [to] avoid becoming conventionalized. Too much reading is a handicap mainly to people who have the wrong attitude of mind. Freshness of outlook and originality need not suffer greatly if reading is used as a stimulus to thinking and if the scientist is engaged in active research (Beveridge, 1957, p. 6).

The GE Experiment

It was in the context of Beveridge's suggestion that I conducted the first experiment in my present research program. My initial question, put simply, was "How is the way you solve a problem influenced by knowing what others have done?" And, as I have suggested, the question has been previously answered in one or another of two seemingly incompatible ways. One answer is that you should know as *much* as possible about how others have tried to solve a problem so that you will avoid "reinventing

the wheel." The other answer is that you should know as *little* as possible about how others have tried to solve a problem so that you will avoid being misled by their preconceptions.

My experiment was naïvely simple. It was based on the notion, already implied by Beveridge's suggestion, that whether prior information helps or hinders you may depend upon its content—what the information actually is, and your attitude towards it—positive or negative. I exposed my subjects to ideas that other engineers had generated in relation to a particular problem. I varied the content of these ideas; I also tried to induce either a negative or positive attitude towards them. I wanted to see how the given information and the induced attitudes affected each engineer's own initial formulation of that problem.

The performance task was based on an actual manufacturing problem. The engineers were to devise an automatic warehousing system that would handle and sort up to 500 different products along a common conveyor belt. The 36 engineers who served as subjects were divided into four experimental groups that were treated as follows:

The Constructive-Homogeneous Condition. These subjects were required to constructively evaluate four overlapping or similar ideas that other engineers had proposed as possible solutions. They spent a total of 20 minutes listing as many advantages for these four ideas as they could. The four ideas were chosen so as to have a common direction—they were variations on the same theme—different ways to identify products by marking the boxes.

The Critical-Homogeneous Condition. These subjects were required to critically evaluate the four homogeneous ideas. They spent 20 minutes listing as many weaknesses for these four ideas as they could.

The Constructive-Hetereogeneous Condition. These subjects were required to constructively evaluate four dissimilar or nonoverlapping ideas that other engineers had proposed as possible solutions. The four ideas were chosen so as to represent diverse directions—positioning the boxes on the conveyor, dielectric constant of the box, use of surface properties of the box, and identification by size and shape.

The Critical-Heterogeneous Condition. These subjects were required to critically evaluate the four dissimilar or nonoverlapping ideas.

Immediately after this evaluation task, all the subjects worked for 20 minutes on their own solution to the automatic warehousing problem. They also worked on a transfer problem, which was included to detect any generalities in the induced effects; this transfer problem required them to think up a new use for a little known physical effect—the Pyroelectric Effect.

The results, in brief, can be stated as follows: Both constructive conditions resulted in solutions that were rated significantly more creative (by

judges who were working with no knowledge of the source of these solutions) than the solutions provided by the critical groups.

Of more interest was the fact that this finding also held true for the unrelated or transfer task. The constructive groups produced solutions that were rated significantly more creative than those provided by the critical groups.

The effects due to information—homogeneous versus heterogeneous—were not statistically significant. The data suggested, however, that the information acted independently of the induced attitudes; homogeneous information tended to facilitate creative solutions on the automatic warehousing problem and heterogeneous information tended to facilitate creative solutions on the Pyroelectric Effect problem.

The University of Oregon Replication

I had to wait until the Fall of 1961 for an opportunity to repeat that GE experiment. By then I was settled in a new location. Because I was now dealing with undergraduates rather than engineers, I had to devise new tasks and alter some other features of the original experiment. But the basic design remained the same. We presented one of two sets of information to our subjects; for each set of information there was a constructive and a critical condition.

Some of the changes from the previous experiment were:

1. The addition of a control group that performed a neutral task instead of evaluating four ideas proposed by other students.

2. The two sets of information differed in terms of commonness rather than homogeneity. The four common ideas were selected so as to be four times more frequent in occurrence among the associations of students than were the four uncommon ideas. Our idea was to see if the constructive and critical evaluation of ideas in terms of their cultural frequency would reinforce or weaken an individual's adherence to the status quo.

3. We included more tasks, such as a judgment task to detect attitude change, to help us get insights as to how the effects of the GE experiment had come about.

Again I will be brief in citing conclusions. Our first analysis suggested that we did not replicate the main features of the GE experiment. Although we employed in the new experiment over four and one-half times the number of subjects that we used in the earlier one, we obtained no significant differences among group means in quality of solutions.

The failure to affect the quality of solutions, however, does not mean that our procedures did not have other effects upon our subjects. The evaluation condition, for example, produced significant effects upon a subsequent judgment task indicating that constructive evaluation tended to produce positive attitudes, and critical evaluation tended to produce nega-

tive attitudes towards the information that was evaluated—the effects, especially in the case of the critical condition, were striking.

The experimental conditions also resulted in significant differences in the context of the solutions. Subjects who were required to constructively evaluate the ideas of others tended to employ some of these ideas in their own solutions. Subjects who were required to critically evaluate the ideas of others tended to avoid using these ideas in their solutions. The reason that these wide alterations in content did not affect quality of solution seems to be more a function of the particular type of task we employed than an actual failure to replicate the GE effects. In the GE experiment quality and content of solution were intimately related. In the University of Oregon experiment, the task we employed—how to induce more Europeans to visit the USA—permitted a large number of possibilities of equal effectiveness.

When we examined the effects of the constructive and critical conditions upon a transfer task, we at first seemingly contradicted one of the findings of the GE experiment. In both the GE experiment and in the new experiment, we discovered a "spread of effect." When the engineers constructively evaluated ideas for the automatic warhousing problem, they later produced solutions to a different, and presumably unrelated, problem (the Pyroelectric Effect) that were judged to be more creative than solutions produced by engineers who had critically evaluated ideas on the automatic warehousing problem. When undergraduates constructively evaluated ideas for the tourist problem, however, they tended to produce solutions to a transfer task that were significantly inferior to the solutions produced by undergraduates who had critically evaluated the same ideas.

We hope that further analyses and information from subsequent experiments will eventually clear up this apparent contrast in the direction of the "spread of effect." One promising lead, for example, comes from further analyses where we divided subjects within each experimental condition into those who reacted one way to the experimental treatment and those who reacted in a different way (we do this in terms of marker variables obtained from tasks specifically put in for this purpose). When we did this, we discovered that the induced attitudes in the "inoculated" subjects produced transfer effects in a direction opposite to the ones we observed for the subjects upon whom the experimental treatments did "not take." We are still continuing these analyses and will want to cross-check them on some of our unanalysed data before we make too much of them, but the results seem promising.

Studies of Induced Change in Initial Solutions

The two examples of my research just described come from the initial phase of my research program. In this phase we were concerned with in-

fluencing an individual's initial formulation of a solution. We were concentrating on the first step, or first cognitive map, that occurs on the initial presentation of a problem.

About a year ago we began experiments in a second phase of the program where the focus is not upon the initial formulation, but on subsequent reformulations of the initial solution. We changed to this new focus for a variety of reasons, one of which is my belief that it is in the reformulation of existing cognitive structures that more can be learned about creative achievement. In other words, the study of how solutions are reformulated seems to bring us closer to my current notion of creative achievement as the recombination or reorganization of ideas that we already possess about a certain situation.

One important advantage of this new focus is that we could now switch from having to compare one individual's solution with that of another to the use of each individual as his own base line. Our new approach involves having each subject first formulate a solution to the performance task; then we introduce experimental variables. He is then asked to reformulate his initial solution. In some experimental conditions we have had as many as three reformulations. Our dependent variable is now always in terms of the amount and kind of change for each individual between his subsequent and original solutions to the same task.

Another advantage, in terms of my interest in preconceptions, is the possibility of doing things to strengthen or to weaken the individual's initial formulation or cognitive structure before we introduce him to new information about the problem. As an example of this phase of our work, I will describe a simple experiment whose purpose was to see what would happen if we had subjects constructively or critically evaluate their first solutions. We had three groups of subjects; those who spent 20 minutes listing advantages of their first solutions; those who spent twenty minutes listing weaknesses of their first solutions; and a control group that spent an equivalent amount of time working on a neutral task. As expected, when asked to reformulate their initial solutions, the subjects in the critical group showed the most change—both in terms of dropping parts or all of their original formulations and in terms of adding new ideas or directions to their second solutions.

My current research is an extension of this latter phase. My theoretical orientation is to look upon the solution of a complex problem as a growth process—as a gradual achievement of a stable and adaptive cognitive map of a particular situation. This growth process involves the breaking down of original parts of the initial cognitive structures and a recombining of these parts into a new and more "satisfying" and "comprehensive" solution.

In line with this analogy, my research assistants and I plan to study the successive reformulations of a solution over an extended time period. We

intend to vary both the number of reformulations and the spacing. As another ramification of this procedure, we want to vary the time in this growth period when we expose our subjects to new information about the problem. My notion is that the same information will have different consequences and effects upon a subjects' final solution depending upon when, in this growth process, he encounters it.

Changes in My Own Outlook

As I have already indicated, my purpose in describing some of my experiments is to show you one attempt to investigate some aspects of the role of preconception in the achievement of a new cognitive map. I have not emphasized the results of such work, chiefly because any of my conclusions would be very tentative. Analysis and digestion of my results proceeds very slowly. I am still redoing analyses of experiments that we ran over a year ago; I still feel that I have yet to command an understanding of the findings that will fully do justice to all their complexity.

As a result of returning to earlier findings in the light of subsequent ones, I keep finding that I have overlooked obvious implications in some of my data because of preconceptions about how data should be pooled and analysed. Some of these preconceptions are those that I share in common with other psychologists because of a particular heritage of psychometric procedures.

My growing awareness of these preconceptions has been accelerated by the occasions during the past three years where I was requested to explain to non-psychologists what creativity research was all about, and how my own work fitted into the broader picture. Out of those experiences, two issues emerged to bother me. I was impressed by the hopes and expectations that non-psychologists have for current research in creativity. And I was surprised, upon looking more closely at the situation, at how ill-fitted are current psychological approaches for dealing with many of the questions that people want to ask about creative thinking.

Limitations of Current Creativity Research

The discrepancy between the kinds of questions we would like to ask about creative thinking and the kinds of questions that current research in creativity is capable of dealing with is, in my opinion, quite large. I think it is important to examine this discrepancy for two reasons. If non-psychologists have wrong expectations about what the current work can tell them about creativity, the resulting disappointment will benefit neither group. And, more importantly from my viewpoint, as long as both psychologists and non-psychologists do not have a clear picture of the kinds of questions that current research procedures cannot handle, then I fear that we may be held back in making those observations and devising the

kinds of approaches that I feel will more appropriately deal with the interesting issues of creative thinking.

My point, in brief, will be that the current tools now being employed by psychologists in their investigations of creativity are quite limited in the kinds of information they can provide. In making this point I do not want you to think that I am overlooking the significant positive contributions that psychologists have made to the field of creativity research, especially during the past decade. In particular, psychological research has brought attention to this much neglected area; it has convinced many individuals that we now can objectively study what had previously been alien to scientific inquiry. The psychologists have introduced a new era in testing, especially by breaking the monopoly of multiple-choice tests; they have forced us to re-examine our concepts of intelligence and they have brought into question our standard ways of indexing giftedness.

Yet, if we are to fully profit from these positive contributions of psychological research, we should carefully examine some features of current methodology that have been quietly taken for granted. We should realize that psychologists entered the field of creativity already committed to standard research designs and fixed routines for collecting and analysing data. These methodological routines, *I cannot overemphasize*, did not arise out of the particular needs and problems of creativity as such. Rather these routines arose early in the history of psychological research in response to other needs and in terms of rather specialized outlooks. Galton devised his correlational method—the one that much of our present work still relies upon—in terms of his specific ideas about the inheritance of fixed abilities which are independent of the environment. And Fisher devised his analysis of variance to deal with problems of agricultural research.

From such beginnings, these tools became gradually more complex and eventally reached the state of codification and ritual that has been frozen into computer programs, textbook dogma and journal policy. When, in 1950, Guilford's Presidential Address to the American Psychological Association initiated the sudden interest in creativity research, the psychologists brought with them into this uncharted domain these standardized tools.

We can look at current procedures in creativity research according to a number of different principles of classification. One important distinction, for example, is that between correlational and experimental approaches. Most of the current research, for example, employs correlational methods. From this standpoint, creativity is looked upon as a property of individuals —it consists of relatively enduring traits and abilities. Some of the more recent research, however, has employed the experimental method. In this latter approach, creativity is viewed as a property of behavior—the prob-

lem becomes that of finding the antecedant conditions or environmental inputs that encourage creativity. The correlational approach, in short, studies creativity in terms of what is relatively permanent or persistent about individuals. The experimental approach places its reliance on what is changeable in human behavior.

Although this distinction between correlational and experimental approaches is a very important one, I want to emphasize a slightly different feature which is currently common to both correlational and the experimental investigations of creativity. This feature involves the distinction between research that deals with isolated components of behavior and research oriented towards understanding the functioning of complicated systems. It is a distinction implied by Lowenfeld's differentiation of "tests" from "performance tasks." In a "test," we isolate one variable at a time for study. In a performance task, we try to simulate the essential features of a complex system so as to study the interactions of a set of variables operating within a unitary system.

With but a few exceptions, the current research in creativity is dominated by classical test theory. Whether we look at the factor analytic studies, the assessment studies, or the experimental research, we see that the ideals of homogeneous or factorially "pure" tests influence choice of observations and dependent variables. The classical test model, in turn, derives from the ideal of research in classical physics—to study a particular variable, you hold all others constant. In pursuit of this ideal, for example, the factor analyst devises simple and brief tests, each one being an attempt to isolate one ability or trait by holding all others constant.

Now, I don't want to be misunderstood. Such a procedure has many potential values. If it succeeds, for example, the assessment research in creativity will help us to replace vague and subjective notions of creativity with objective and communicable descriptions. It is an entirely empirical question as to whether the current research can ultimately replace current global classifications of creativity with sets of objective and easily communicated descriptive dimensions. And, in this endeavor to improve the descriptive basis for classifying states of creativity, we can only hope that success will be achieved. Although I am frankly skeptical as to how well current assessment approaches can succeed with simple linear models and independently isolated traits, I have to agree that life would be immensely simplified if we can get along with classification procedures based on current assumptions.

But, having made clear the potential contribution of the current methods, we must also emphasize what they cannot do. By their very nature they cannot deal with the interaction of variables in a system. A creative achievement, for example, is the outcome of a *combination* of variables

operating and interacting as part of a unitary system. The process by which this achievement is reached involves a hierarchical organization of dependencies and interactions of different operations. Only if we assume that creative achievements result from the operation of extremely simple systems—systems in which the output is the simple additive sum of separate and isolated components—can we hope to employ the results of current creativity research to help us understand creative achievement.

Although creative achievements are obviously the product of the ways in which individuals *combine* operations in pursuit of a goal, the current methods of creativity research deliberately remove this feature from the behavior they deal with. In dealing with test behavior rather than behavior on performance tasks, the current procedures emphasize the complexity of creativity in terms of a number of different operations on decentralized tasks. Each operation, or assumed component of the creative process, is measured on a task in which all other components are held constant. Even in the few cases where the components or operations are measured in a more complex task, the use of linear combinatory models in the summarization and analysis of the data removes interaction or nonlinear constraints among the components. As Ashby has pointed out in another context: *"The defining of the component parts does not determine the way of coupling.* From this follows an important corollary. That a whole machine should be built of parts of given behaviour is not sufficient to determine its behaviour as a whole: only when details of coupling are added does the whole's behaviour become determinate." (Ashby. *An Introduction to Cybernetics.* 1958.)

In starting with test behaviors and in using the linear combinatory models of current factor analytic, multiple correlational, and analysis of variance designs, I believe that it can be demonstrated that the overwhelming majority of today's investigations of creativity do not deal with the "details of coupling" among component parts. Rather, both their virtues and weaknesses are intimately tied up with the fact they are basically methods for isolating and cataloging components of behavior. From this fact, stem many consequences. I will merely point to a few of these in the remainder of this paper.

The first consequence is the tendency to confuse a test behavior with its analogue in a performance task. If the factor analysis reveals a "factor" of originality and another one, say, of evaluation, then the implication is that these factor abilities are in some sense like their counterparts that occur in performance tasks.

Although I am not questioning the utility of factors or test abilities in classifying or discriminating individuals from one another, I do feel it necessary to warn against confusing factorized abilities with their anal-

ogues in actual performance tasks. The factorized abilities are abstracted from tests which are deliberately contrived to constrain the testee to respond in isolation from his other abilities and from normal goal-seeking behavior. It is like measuring the components of a servo-mechanism in isolation from the remainder of the system to which they belong. Just as we cannot understand the contribution of a component and its function within a system from its behavior when isolated from that system, we should not allow ourselves to be seduced into assuming we understand the behavior of such things as "cognition," "retention," "divergent thinking," and "convergent thinking" in the thinking process through information that is derived from tests which isolate these from their normal system-functioning.

Related to this possibility of confusing the behavior of isolated components with the analogous components within a system is the mistake of assuming that the assessment method and factor analysis can tell us anything about creative thinking as an adaptive, goal-seeking process. In fact, these procedures can tell us little about thinking as a process or system. This should be clear once we reflect upon the fact that the assessment procedures rule out system-behavior right from the start. The very model and method of data reduction which is the heart of these methods removes that aspect of adaptive behavior which involves sequential dependencies among the various activities and their interactions with the environmental outcomes that they bring about.

It is the interplay among the separate types of activities that an individual can perform, the feedback from each activity, and the regulatory responses of the individual to the outcomes of his prior activity in terms of the guiding goal that is the hallmark of creative thinking.

Rather than systematically running through other limitations in the current psychological tools being used in creativity research, let me merely list a sample of some approaches that I believe would not occur, or be allowed to occur, to anyone who is thoroughly steeped in assessment and psychometric procedures.

1. *The Process Tracing Experiment.* Some of the classic studies on problem-solving, such as that of Dunker and Ruger, tried to deal directly with thinking as an adaptive and goal-directed system. In such cases, it made sense to present the subject with a problem and then allow him free rein to marshal and organize his forces in what ever way was "natural" for him. One of the early studies in creative thinking also used this process-tracing approach. Patrick used the procedures both with artists and poets as well as laymen to get some insights into the stages of the creative process. The present approaches to creative thinking have rigidly avoided

process-tracing experiments. In my opinion, they have done so for two reasons:

a. To the modern investigator it doesn't make "sense" to investigate phenomena or to collect observations that do not easily lend themselves to modern computing routines and to standardized rituals for data analysis.

b. The assessment procedure, and especially factor analysis, seems to place great value on what is called "factorial purity"— the goal is to use tests that are individually homogeneous and are not complicated by the possibility of more than one type of behavior or approach in a given task.

Yet, in spite of the seeming desirability of "factorial purity" and of the apparent fulfillment of the canons of measurement displayed by the factorial and assessment studies, it is interesting to keep in mind that the psychologists who are trying to simulate human thinking by means of a computer program use the classical studies of process tracing for guidance. You cannot simulate thinking with factors, traits, or abilities.

2. *Lowenfeld's index of the "one best performance" from each subject.* I once read a talk by Lowenfeld where he discussed his notion of "creative intelligence"—the idea that different individuals could be aware of their own limitations and the fields in which they could best perform. As one consequence of this concept, he reanalysed some data where he used as the one index for each subject his best rating on one of several different performance tasks.

When I first read about this my psychometric instincts caused me to recoil. You just can't do this! It violates several sacred principles of measurement and psychometrics! Yet, after I calmed down, I realized that Lowenfeld's procedure does make sense if we believe in something like "creative intelligence." And, on further reflection, we can also see the built-in blinders that keep psychometricians from thinking of taking such a step. Our tacit acceptance of computing routines without full realization of their implications also tends to seduce us into confusing our indexes with the constructs we hope they are representing. Yet, it makes perfectly good sense that the same construct might be best indexed with a different performance from different subjects.

3. A similar possibility for treatment of certain data has recently occurred independently to me and two of my colleagues at the University of Oregon. Interestingly enough, each of us had been thinking of the idea for quite a while, but had not discussed it with each other or colleagues because we were almost certain that it would meet with ridicule or with the criticism that it violates the most cherished assumptions of psychometric measurement. This idea, related to what I have already spoken about, is that an experimental effect might reveal itself in a number of different modes. And this mode might differ with different subjects. As an example: I have

been systematically collecting data, in all my experiments, on not only induced changes in quality of solution, but also in content, on transfer, on attitude change. If a subject shows a change in any one of these above a certain cutting point, I intend to score him as showing an "effect." The point is that something like this will not typically occur to a psychometrician.

Conclusions

I started my paper by saying that I was concerned with the role of preconceptions. In particular, I view creative achievement, in large part, as the overcoming of preconceptions in order to more fully utilize the implications of what is already "known." I described, in brief, some of my research strategy in trying to experimentally investigate some of the determinants of creative achievement—especially the role of the prepared mind. In the course of these investigations, I gradually found myself becoming aware of the need to violate principles and assumptions that I had taken for granted. That is, I had to violate these preconceptions if I was to adequately do justice to the phenomena that seemed to be occurring in my laboratory. It was this situation that led me to examine the preconceptions that I share with my psychological colleagues. In particular, I wondered how such preconceptions might be limiting our outlook in current creativity research.

The conclusion I reached, and tried to communicate to the reader, is that whatever their merits, the current psychological investigations of creativity cannot deal with a large number of questions that we might want to ask about creative thinking. And some of these questions, I would suggest, are among the most interesting ones that we could ask about creativity. My reason for emphasizing the limitations of my own craft is that I fear that a failure to understand what current psychological research cannot do may have two consequences: it may keep us from adequately making use of the kinds of information that are currently possible from such procedures; and it may impede us from going off in new directions, directions which are more directly dictated by the nature of the phenomena of creativity rather than the bulit-in assumptions of current psychological tools.

Psychologists and statisticians have not been blind to the limitations of their current procedures. In the area of experimental design they have been devising new methods such as analysis of covariance, multivariate analysis of variance, multiple comparison techniques, new kinds of trend analyses to better adapt statistical tools to psychological complexity. In the area of assessment and factor analysis we have oblique factors, simplex and circumplex models, canonical correlations, multiple discriminant functions, and other changes to take advantage of the computer age as well

as to break out of the overly-simplified models implied by the original correlational and factor procedures.

But these changes have their dangers. They do not really get rid of the preconceptions that I have already mentioned. In many cases they add even more preconceptions. Furthermore, they complicate an already serious problem—that of separating the investigator from an intimate contact and appreciation of his data. If we plot a graph in a simple two-variable experiment, where both co-ordinates have a simple observable referent, the resulting picture can usually convey an intuitive grasp and appreciation of the underlying relationship. But when our data gets reduced and transformed to canonical variates, factors, latent roots, inverted matrices and other esoteric by-products, we simultaneously throw out our intuitive comprehension of what is going on. The gaps between the raw data and the end product are filled with huge, unintelligible voids. Each of these voids may hide certain preconceptions and decisions that affect the possible outcomes in the data.

The most serious danger in this growing complexity of statistical designs is what I would call an "aloofness" from phenomena. Most of the developments and complexities in statistical methodology have been designed to handle data after they have been collected. Little or no improvement has been made at the observation end. In fact, the prestatistical psychologist was probably in a better position for getting insights and adjusting his behavior to fit the phenomena than is his modern counterpart. Today's psychologist has already committed himself and his data to a fixed number of ways in which it can express itself before he even collects it. In many ways he imposes a structure of orthogonality and linearity upon nature. Rarely is he in a position to let nature speak for herself. If nature's figure is shaped other than what is implied by a linear set of orthogonal and weighted components, many psychologists will never have the opportunity to observe this fact.

The Moral

What is the moral of this story? In part, I think it is contained in a letter that Pavlov wrote to the youth of his country in his 88th year. The letter is quoted in a very stimulating talk that Robert S. Morison, of the Rockefeller Foundation, gave before the American Psychological Association in 1959. Pavlov urged:

"Firstly, gradualness. About this most important condition of fruitful scientific work I never can speak without emotion. Gradualness, gradualness, and gradualness. From the very beginning of your work, school yourselves to severe gradualness in the accumulation of knowledge. Learn the ABC of science before you try to ascend to its summit. Never begin the subsequent without mastering the preceding."

In other words, in the study of creative thinking, maybe we should take first steps first. And before we move on to each new step, let's make sure that we have thoroughly mastered and digested what went before. If we are going to be enticed into using the psychometric tools of correlation, standardized tests, factor analysis, multiple correlation, discriminant functions, t-tests, analysis of variance, and the like, let us make sure that we have a realistic grasp of just what we have purchased. I would strongly urge you to never use a tool whose full implications are unclear to you. If you really do not have a full grasp of what factor analysis is and what it can and cannot reveal, then I say you should not use it.

I think there is a strong case to be made for naïveté in the study of creative thinking. I think there may be wisdom for the non-psychologist who is interested in creativity to turn away from the psychologist and psychological methodology and, rather, turn towards the phenomena of interest. There still is hope that the non-psychologist can look at the phenomena with non-linear and uncommitted eyes. And in so doing, he may still possess what Bruner has termed the "freedom to be dominated by the object."

The case of the ethologists is relevant. The work of such ethologists as Lorenz and Timbergen in animal behavior has caused a major revolution within the conceptual system of modern psychology. Their work has led to major reexaminations of our notions of learning, instinct, heredity, early experience, and species differences. How was this revolution accomplished? By the use of naïveté. The ethologist began with no preconceptions except the notion that it would be wise to look at what an animal does in its natural habitat—to collect observational data on the temporal sequence of various kinds of acts in the life of an animal. They were especially interested in contingencies of behavior—what follows what and in response to what. With such a humble beginning, the results have had repercussions which are still shaking the foundations of traditional psychology.

Maybe what we need is an ethology of creative behavior. Maybe what will provide the breakthroughs and the challenge to the current orthodoxy of method in the study of creativity, will be what emerges when dedicated people decide to look carefully and systematically at creative phenomena with a minimum of commitments and with no ready-made preconceptions about how to collect and analyse the data.

In other words, in the present state of the art, it may be the non-psychologist, such as people in art education, for example, and others who are close to the phenomenon they are interested in, who will supply us with the starting point. At this beginning level, only the crudest techniques are necessary—simple graphing of data, etc. In this way, perhaps,

as we gather a good observational store and as our picture begins to build up, we can begin to think in terms of methodological procedures that more adequately represent the actual forms of the phenomena.

Again, I don't want to be misunderstood at this point. I am not advocating that the results of current research are useless. On the contrary, I believe that they embody many important regularities that will have to be taken into account in a more comprehensive approach to the problem. What I do claim, however, is that whatever utilitarian and classification values the outcomes of current research may possess, their significance and interpretation can only be gauged by approaches that study *systems* of behavior. Only by such studies of functioning systems can we adequately judge which of the findings from current procedures play a role, and in what manner, in actual creative achievement.

Nor am I advocating a retreat from rigor or quantitative methods. Instead, I would argue that our quantitative procedures should be relevant to the phenomena we are studying—they should emerge as a consequence of the actual nature of the phenomena under consideration. The current models and approaches actually impose a structure upon the phenomena to be observed. Investigators have prematurely decided upon kinds of measurement procedures to employ and have also selected the types of observations to make to fit the needs of their procedures. This, in my opinion, is not unlike coaching students on the questions of an intelligence test as a way of raising their intelligence. In other words, I think that current research in creativity has attempted to begin with quantification and rigor and end up with understanding; whereas I would argue that the natural sequence should be understanding first and quantification and rigor emerging as a corollary of this understanding.

By following Pavlov's advice, by going at it gradually and step by step, perhaps we will achieve that ideal of being able to utilize the fruits of our past labors to gain a more accurate understanding of creativity. The current limitations are not what we do not know about creativity; the limitations are in what we are taking for granted in our attempts to learn more about creativity.

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APPENDIX

In this appendix I want to amplify some of my remarks, partly as a result of questions raised by discussants and other individuals who have commented on the paper or the original draft of the paper. I also would like to indicate sources which express viewpoints similar to some that I have voiced in the paper.

1. After having prepared the paper, I was gratifyingly surprised at the extent to which my comments had been anticipated by other writers in a variety of different fields. Here, I will list just a sampling of some of these sources:

a. Back in 1930, Kohler attacked both Behaviorism and Introspectionism in his classical work *Gestalt Psychology*. (Revised Edition. New York: Liveright, 1947.) Much of his critique, with little alteration, can be validly aimed against the current work in creativity. Kohler points out that use of indirect methods of measurements in physics was preceded by a long history of cruder, but more direct contact with the phenomena of interest. Cavendish, for example, used his own subjective reactions to calibrate the intensity of shocks. "Where in psychology," asks Kohler, "have we that knowledge of important functional relationships on which indirect and exact measurements could be based? It does not exist. Therefore, if the development of more exact methods presupposes the existence of such knowledge, the gathering of it must be our first task. For the most part, our preliminary advance in this direction will have to be crude. . . . If we wish to imitate the physical sciences, we must not imitate them in their highly developed contemporary form. Rather, we must imitate them in their historical youth, when their state of development was comparable to our own at the present time." Later he points out that "too great an interest in available quantitative methods is not a promising state of mind at a time when the development of psychology depends upon the discovery of new questions rather than upon the monotonous repetition of standardized methods." Kohler makes several other observations that anticipate in many ways my current comments about how current research blinds itself to the important issues by its manner of selecting observations and reducing the data in terms of overly restrictive models.

b. Kurt Lewin, of course, with his emphasis upon field theory in psychology, has much to say that overlaps with my present paper. His methodological critiques, such as his contrast between the "Aristotelian" and "Galilean" modes of thought in contemporary psychology, seem relevant here. (Cf. Lewin, K. *A Dynamic Theory of Personality*. New York: McGraw-Hill, 1935. Also, Lewin, K., *Field Theory in Social Science*. New York: Harper, 1951.) Especially pertinent is Lewin's strong emphasis upon a dynamic, constructive approach rather than a methodology that relies on abstract classifications.

c. Ludwig von Bertalanffy has been a leading exponent among the biologists of the need to study the functioning of living systems as opposed to the study of isolated components of these systems. In his book *Problems of Life* (London: Watts, 1952) he marshals an impressive array of concrete examples in biology to convincingly make the point that even the simplest biological organism can not be understood in terms of classical models and methods of physics.

d. In the area of sociology, my colleague, Professor Robert Dubin, was kind enough to lend me a draft of a forthcoming chapter that he is contributing to

a book on *Theory Building*. His chapter, which is entitled "Process and Outcome in Theoretical Models", makes an interesting distinction that is relevant to the current paper. He feels that current research in sociology can be divided up in terms of one of two goals—that of "understanding" and that of "prediction" of outcomes. He makes a convincing case that each of these goals does little to contribute to the other. In his terminology, I would classify current efforts in creativity research as oriented towards the prediction of outcomes. And, as he implies in his discussion of the "precision paradox", it is possible that the greater the success that current methods might achieve for predicting outcome in particular situations, the less contribution such research might have for helping us understand the interaction of variables in the process of creative thinking.

2. How do we go about implementing a research program that is oriented towards the study of how an individual combines operations in trying to achieve a more satisfactory cognitive map of a complex situation? In more general terms, how do we go about studying systems, especially ones that may be non-linear, as opposed to the current approaches which emphasize isolating and measuring one component at a time?

We can take our cue from several sources. In engineering, for example, the study of non-linear systems has recently taken on great importance because of their occurrence in performance of missiles, airplanes, the determination of weather, the flow of streams, and the design of electric circuitry. Where he cannot handle the problem with linear equations, the engineer builds miniature models, or he simulates the complexities of his system on the computer. He has learned through experience that, before he can reduce the performance of a complex system to a mathematical model, he must first study it as a unity; he must measure its performance under conditions where its several components are operating simultaneously and under constraints imposed from without and by the interconnections within the system.

Ashby, in his book on cybernetics and in the one on *Design for a Brain*, also shows us how we can approach our problem and still maintain rigor and quantification. A very instructive account of how we can study a complex system by simplifying some aspects, but still retain essential interactions among component parts, within the laboratory situation is demonstrated by Professor Park's recent report on competition among populations of beetles. (Park, T. "Beetles, Competition, and Populations." *Science*, 1962, 138, 1369-1375.) Knowledge of the conditions under which each of two populations of beetles flourish optimally, is not sufficient, for example, to anticipate which of the two populations will survive when put in competition with each other under varying conditions.

In the area of creativity research, Patrick's work on studying the stages through which artists and poets proceed as they actually create a work of art is a step in the direction that I am advocating. Some of the recent work of Hoffman and Maier on the effects of group composition and the creativeness of problem solutions also illustrate how we can do experiments using tasks that are the outcomes of combinations of operations. In their studies they employ realistic problems, some of a human relations type, involving role playing. (Hoff-

man, L. R. "Homogeneity of Member Personality and its Effect on Group Problem-solving." *Journal of Abnormal and Social Psychology*, 1050, 58, 27-32. Also, Hoffman, L. R., & Maier, N. R. F. "Quality and Acceptance of Problem Solutions by Members of Homogeneous and Heterogeneous Groups." *Journal of Abnormal and Social Psychology*, 1961, 62, 401-407.)

3. In his discussion of my paper, Professor Manuel Barken quotes Hayakawa, as I recall, as follows: "Every language leaves work undone for other languages to do." I would add that the current models underlying creativity research leave work to be done by other investigators, but I suspect this work will not be done until we make it clear what is being left undone by the present approaches.

4. Finally, I cannot resist the temptation to quote from a recent article by a former chemist who has turned to the study of living phenomena:
"With all these limitations and difficulties on the molecular level, partly theoretical, partly experimental, one becomes suspicious. Could it be that the conceptual and experimental restrictions in which we find ourselves when investigating phenomena on the molecular level, make the molecular approach unsuitable to explain phenomena evident on the level where the individual biological entities can be observed, and for this reason the phenomenon of directiveness eludes us? Is it possible that our thinking in physical science is so much restricted to molecular and statistical approaches that we find ourselves unable to treat physico-chemically the actual difference between biological and non-biological phenomena, exactly because of this restriction? We have seen that, when observation of a single biological entity (such as a virus) is possible, it is still possible to accumulate empirical observation which shows a directiveness and an unusual efficiency of biological processes to an extent which never manifest itself in purely chemical processes. But, once the observation is on the molecular and statistical level we cannot *see* such efficiency." (Mora, P. T. "Directiveness in Biology on the Molecular Level?" *American Scientist*, 1962, 50, 570-575.)

5. My colleague, Professor Richard Littman, doubts whether one could call Lorenz a "naive observer". He suggests that the combination of a strong Kantian bias plus training as a naturalist prepared him to look at behavior in a way that differs from contemporary psychology. Because of Lorenz's strong biases, psychologists have not taken his explanatory models very seriously, but the new phenomena and concepts that his observations have brought up, nevertheless, have started a revolution in both biological and psychological thinking whose full consequences have yet to be realized.

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